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ABSTRACT

A seminar paper concerning the relationship between applied and basic research in the study of educational and social inequality, and two commentaries are presented. The paper--"In Defense of Ivory-Towerism: Confessions of an Unreconstructed Basic Researcher," by Karl L. Alexander, the seminar's main speaker, presents the point of view of a sociologist of education whose major focus is educational stratification. It reviews the basic research findings of issues of educational and social inequalities, within a discussion of the possible contributions of basic research to the solution of educational problems. Areas addressed in the research review include the Blau-Duncan "basic model," the Wisconsin School Process Model, studies of gender and racial disparities, the social-psychology of the schooling process, school organization and climate, and family factors. It is concluded that fundamental research can help to locate the role of the school in the larger social system, suggest strategies for improvements within the school environment, and combine an inward and outward approach to shed light on the interdependence of school and society. The commentaries include: "Comments from Another 'Ivory Tower'" (Philip J. Foster), and "Applied and Basic Research in the Sociology of Education: Comments on Karl Alexander's "In Defense of Ivory Towerism"" (W. Paul Vogt). (TJH)

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BASIC AND APPLIED RESEARCH ON EDUCATION AND SOCIAL INEQUALITY:

Proceedings of the New York Education Policy Seminar

Karl L. Alexander
Philip J. Foster
and
W. Paul Vogt

November 1988

Presented by
The School of Education
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and
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State University of New York

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Proceedings of the New York Education Policy Seminar

BASIC AND APPLIED RESEARCH ON EDUCATION AND SOCIAL INEQUALITY

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MAIN PAPER:

IN DEFENSE OF IVORY-TOWERISM: CONFESSIONS OF AN UNRECONSTRUCTED BASIC RESEARCHER 1

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Department of Sociology
The Johns Hopkins University

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APPLIED AND BASIC RESEARCH IN THE SOCIOLOGY OF EDUCATION: COMMENTS ON KARL ALEXANDER'S "IVORY TOWER" 55

by W. Paul Vogt
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EDITOR'S INTRODUCTION

It is my great pleasure to introduce the Sixth Annual New York State Educational Policy Seminar, which is jointly sponsored by the School of Education at SUNY at Albany and SUNY's Rockefeller Institute of Government. Today's topic is the relationship between applied and basic research in the study of problems of educational and social inequality.

We are especially fortunate to have as our main speaker Dr. Karl L. Alexander. Dr. Alexander is Professor and Chair of the Department of Sociology at The Johns Hopkins University, where he also holds a research appointment at the Center for the Social Organization of Schools. He is among America's most influential sociologists of education conducting basic research on topics of great policy import, particularly on issues of educational stratification. His paper today is entitled "In Defense of Ivory-Towerism: Confessions of an Unreconstructed Basic Researcher." It is an exceptionally informative review of the basic research findings of issues of educational and social inequalities, all set in a context of a discussion of the contributions basic research may make to efforts to solve educational problems. As Dr. Alexander puts it, his paper address the question of how basic research on educational stratification "which makes no pretense to change things might be of service to those who want to make schools better."

Following Dr. Alexander's paper, Professor Philip Foster and I will offer brief critical commentary. The first commentary will focus on Dr. Alexander's treatment of issues of education and social stratification, the second on the relation of basic and applied research.

In Defense of Ivory-Towerism: Confessions of an Unreconstructed Basic Researcher

Dr. Karl L. Alexander
Department of Sociology
The Johns Hopkins University

Introduction

Before getting into the substance of this essay, I need to tell you a bit about myself. I hope this won't seem too self-centered, but it's important to setting the stage for what follows. I've been involved in education research since 1972, when I finished my Ph.D. and took my first professional position in the then Department of Social Relations at Johns Hopkins. I've been at Hopkins my entire career, and during most of my time there I've worn two professional hats, one as a tenure line member of the Arts and Sciences faculty, the other as a research scientist at the Hopkins' Center for the Social Organization of Schools. From this institutional base, I've been in the privileged position of being able to pursue my interests pretty much wherever they took me. I think it's fair to say my time at Hopkins has been reasonably successful, at least as judged by conventional standards. I've managed to publish a good bit of original research in respected outlets and now hold the lofty title "Full Professor," which carries (I hope) lifetime job security. My first confession: After nearly 20 years of hard work, expenditure of a small fortune in grants from numerous public and private funding agencies, and receipt of many rewards for the effort, so far as I know not a single school has altered its practices and not a single student has had his or her situation improved as a result of my studies, at least not as a direct result. My second confession: I am not particularly troubled by this lack of demonstrable impact of my life's work on happenings in the real world.

I would add immediately, though, that I think my research is important, and useful, despite its being several steps removed from practice. My purpose in this essay is to tell you why I think this. Actually, and more properly, it is to make a case for the kind of work I do: discipline-based fundamental research on topics of educational relevance.

At first blush, this might seem a rather odd agenda. We all know that the academy values knowledge for its own sake, and it wasn't too many years ago that professorial types as a class would sneer derisively at so-called "action research" (see Gollin, 1983, for a description of this attitude in sociology). But much has changed in recent years, and it is my impression that circumstances have combined to put fundamental research on the defensive, at least in some circles. For one thing, it's no longer the only game in town. Academics can still look with disdain on the beltway bandit, fee-for-service research shops that ring D.C., but there are a good many talented people involved in such enterprise and it represents an impressive nonuniversity-based research capability.

Like it or not, policy-oriented studies have become respectable. This is reflected in the explosion of degree-granting programs and of areas of specialization in traditional programs that presume to tackle the formulation, evaluation, and implementation of policy head-on. Correspondingly, there has come into being a literature, complete with texts and journals, that seeks to advance a methodology for doing applied research, including so-called "evaluation studies" (see, for example, Cronbach et al., 1980).

Of course, none of this has happened in a vacuum. To a considerable extent, these developments have been driven by the voracious appetite for information of an activist federal government struggling to do battle with many problems on many fronts. This began on a grand scale during the sixties, and has characterized every administration since, regardless of its party affiliation or nominal philosophical persuasion. Despite wide swings in both levels of funding and funding priorities, the social sciences have carved out a niche for themselves as part of the government's Standard Operating Procedure (SOP). So long as there are problems defined as social, the social sciences will have a role to play. This is the essence of "legitimation," or even of "institutionalization"; both are apt characterizations. It also has become very big business.

But why didn't academic researchers simply wallow in this largesse? Part of the answer, I think, is that the university-based research infrastructure was overwhelmed by the sheer magnitude of this new demand for the kind of work it did. Various considerations limit the university's ability to expand, contract, and reorient its research capability in response to short-term funding swings. Not the least of these is the university's instructional mission, and pressures for teaching-research integration. Sole-purpose private sector research firms suffer no such constraints.

And too, the character of this "demand" often was, and remains, a problem for academic types. The incompatibilities here are both intellectual and professional in nature. On the intellectual front, classically trained scientists, social and otherwise, subscribe to a philosophy of knowledge that makes them seem rather like oddballs in the real world of problems in need of solutions—their belief that knowledge is always tentative, that proof is something ever to be approached but never realized, etc. What follows is an inclination toward caution that often is mistaken for lack of conviction and derided by people of action. Social scientists steeped in this tradition often could not, or would not, provide the kinds of answers wanted by policymakers, practitioners, and the general public.

These intellectual "failings" are exacerbated by a disinclination on the part of most academics to be treated like employees. "Pay our bills, then leave us alone" is the prevalent attitude, and I believe a strong case can be made for indulging such self-indulgence, if only selectively. But this mentality sets up interference in exploiting the academic's considerable talents in the policy research arena. Academics don't take kindly to having others tell them what they should be studying, how they should execute their studies, and how they should comport themselves along the way. All of this is anathema to the classical vision of how scholarly inquiry should proceed, yet policymakers

have specific information needs, the bureaucracy lives by rules and timelines, and the public is impatient for solutions. Traditional academics are constitutionally incapable of entering into such partnerships, or at least of honoring their commitments under them. It is none too surprising under such circumstances that those who write the checks should cultivate other sources of advice, information, and, yes, research.

I hope this abbreviated account is not received as either demeaning or trivializing the movement toward applied social sciences. It surely is not simply a matter of some new or otherwise unmet need calling forth its solution, as a simple-minded functionalist account might have it. I appreciate that many talented individuals, frustrated by "pure science" blinders, have labored mightily over many years and obstacles to bring into being an alternative, or at least complementary, vision of social science practice (much of this history is reviewed in Lazarsfeld and Reitz, 1975, and Gollin, 1983). I respect, and applaud, these efforts. Yet I also believe that the "moment in history" has had great bearing on the movement's impressive successes.

Relevant too are assorted frustrations with the basic science approach that go beyond those mentioned above. For one thing, competition not only exists, but it can be awfully stiff as well. Consider the case of public opinion polling, arguably the crowning accomplishment to date of the applied social sciences. Advances in our ability to tap the public pulse have been nothing short of revolutionary. As a result, we are bombarded daily with near instantaneous, and in the main remarkably accurate, answers to all sorts of important (and not so important) questions. This is a far cry from intervening to right a wrong, but it is what most people see of the social sciences and it is evidence of a technology that can be deployed effectively and efficiently to meet real needs. Against these demonstrable successes, and the high expectations they encourage, the basic researcher's plea for support to pursue esoteric interests in the hope that a possible future breakthrough could conceivably have practical spinoff rings awfully hollow. Combine this with a sense of urgency as problems seem always to grow worse, understandable bottom-line pressures to weigh yield against investment, and the occasional smear in the press (e.g., the infamous Golden Fleece Awards) that conveys the basic sciences as frivolous, corrupt, or both, and what you wind up with can be a siege mentality.

The environment, it seems to me, clearly has changed, putting a basic science approach to knowledge cumulation on the defensive (see Dornbusch, 1970-71 for a counterassault). Perhaps this is too sweeping, but I certainly think it holds for basic science research in areas of the social sciences that deal with things considered "problems" and for which there are large, nonacademic constituencies. The educational arena satisfies these conditions, and indications of disaffection with the basic research approach are, unfortunately, all too available.

For example, writing in a recent issue of *Educational Researcher,* Assistant Secretary of Education Chester Finn (1988, pp. 5-8) writes, "our labors haven't produced enough findings that Americans can use, or even see the use of. . . . Education research has not fulfilled its role in the

effort to improve our schools." And Richard Shavelson felt moved to devote his Presidential address at last spring's AERA annual meeting to the topic (published in *Educational Researcher*, Oct. 1988), laboring mightily to point out some overlooked contributions. I recommend Shavelson's paper to you, for it makes many worthwhile points. But it also includes the following assertion:

> I want to make clear that education research can be justified as legitimate inquiry in its own right. We do not have to prove its worth on the basis of improving educational practice. . . . Indeed most education research bears on theory or a particular line of empirical inquiry, *as it should*. I see no reason for us to rush out to be relevant!

I agree wholeheartedly with this sentiment, but it is too simple in today's environment to simply assert our claim and leave it at that. When our research is expensive, as it is, when it uses other people's money, as it does, and when resources to support education fall far short of what is needed, as, sadly, they always seem to, then it is incumbent on those of us who share Shavelson's convictions to at least flesh out the claim, so that others can better judge its cogency. This is the task I set for myself in the pages that follow. Let me begin be defining my terms, so it is clear what I intend to include under the rubric "discipline-based fundamental research."

By "basic" or "fundamental" research I have in mind the kind of scholarship Lindblom (1984) chose to set aside in his earlier lecture in this series on principles of policy research: "The science for its own sake social scientist who ignores practical problems and simply follows his or her own curiosity." This exclusion made perfectly good sense in light of Lindblom's purposes. It is unfortunate, though, that he chose to juxtapose the "following of one's curiosity" with "ignoring of practical problems." The basic researcher's curiosity has to come from somewhere, and social concern certainly is fair game so long as it ties in with a disciplinary agenda. And to "ignore practical problems" does not necessarily mean that one's work will be irrelevant to practical problems.

By "discipline" I have in mind one of the traditional arts and sciences fields of study. My own disciplinary background is sociology, but psychology, social psychology, economics, anthropology, social anthropology, and history all can claim large, and useful I think, education literatures. By "discipline-based" I mean research whose questions are framed within a disciplinary perspective. In practice, this usually means working within a conceptualization or theoretical framework whose lineage is disciplinary, *and is not education specific*. There are no further exclusions or restrictions with respect either to perspective or method, although my own studies have been all survey based.

By "fundamental" or "basic" I mean research whose objective is to inform understanding of the disciplinary issues which constitute the study's backdrop. The goal, then, is to test or refine theory or to learn new facts that might have bearing upon the orienting perspective. Importantly, it is not to improve practice, at least not in an immediate, "hands-on" sense. This I take as the distinguishing feature of applied/action/evaluation/policy studies, terms which I here am using

interchangeably. (These distinctions are discussed at length in many sources. See, for example, Etzioni, 1978; Rossi and Whyte, 1983; and Giles-Sims and Tuchfeld, 1983, on the basic-applied distinction and on varieties of applied activity in sociology.)

At the extremes, the distinction between these two research styles is reasonably clear. Program evaluations that are intended to determine whether some aspect of practice is doing what is expected of it, or that are designed to help those responsible for making policy select between alternative ways of accomplishing a particular learning objective, are meant to inform practical decisions. At issue is what works, or which works better. At the other extreme are studies that test some proposition about human motivation, attitudes or the like, in which the school as the setting and the student as the subject are entirely incidental—matters of convenience, rather than integral features of the problem. Much of what we think we know about the social-psychology of late adolescence and early adulthood derives from research of this sort, knowledge perched precariously on the shoulders of an army of undergraduate psychology majors.

Short of the extremes, the distinction often blurs. Reliance upon disembodied theory is an insecure guide, as the best program development research will be theoretically grounded. And one often finds guarded assessments of possible practical implications in even the most basic of basic research. Intent and context can help resolve ambiguous cases, but I suspect there always will be a grey zone. With respect to intent, the main distinction is whether or not the research is intended to provide information that will enable responsible actors to do things better *at the operational level.* Often this will be reflected in sponsorship by practice agents or agencies.

However, since the practical import embedded in knowledge is highly variable, and since many fundamental researchers no doubt hope that what is learned from their studies will somehow transform society, "intent" alone is insufficient. I would add the further stipulation that the research be conducted in a development context, as in "R & D." So, for example, the early literature that searched for instances of exemplary or exceptional schools in order to understand the reasons for their success would not qualify as "basic" because it stopped at knowledge generation (e.g., Rutter et al., 1979). In contrast, attempts to put this information into practice by developing, and evaluating, strategies for making "ordinary" schools more like exceptional ones would constitute an applied extension of this basic research agenda (see, for example, Brookover et al., 1982, and the special issues of Brandt, 1982, and of Bechel, 1983, devoted to the "effective schools" movement).

Ambiguous cases are inevitable, as the "basic"—"applied" distinction, if there is merit in it at all, surely is one of degree. Some of my own work, for example, gets precariously close to an operational agenda for improving particular aspects of schooling. As an example, I've done a good bit of research on the high school curriculum, and one of my studies shows that students who take more high-level math and science courses in high school and perform at a level of "B" or above in them reap substantial benefits in terms of improved quantitative skills (Alexander and Pallas, 1984; see also Jones, 1987). Another study that similarly focuses on curriculum as a source of leverage

shows that much of the male-female gap in quantitative performance at the end of high school can be attributed to differences between boys and girls in course-taking patterns, as boys' programs tend to be much more quantitatively loaded (Pallas and Alexander, 1983).

The knowledge that performing well in a sensibly designed program of study can boost achievement certainly helps, but it's hardly the entire story. Such studies are framed in the basic science context of understanding organizational process. As such, they lack a blueprint for implementation that offers any prospect for success. Such a blueprint, for instance, would have to make allowance for reproducing the "natural" conditions that engage students' energies and interest as curriculum requirements are upgraded. In my view, this neglect of "implementability" issues is what makes my research "basic" rather than "applied," and I am quite comfortable with this criterion as the final arbiter of uncertainties.

Having thus set the stage, I'd now like to consider some of the ways in which "discipline-based fundamental research" can be useful. I'll do this by way of personal illustration, drawing mainly on my own work and the research literatures to which it has contributed. Since I consider the case study embodied in my research career reasonably typical, it is my hope that this somewhat self-centered exercise will suggest some general principles.

From Social Stratification to the Sociology of Education: A Brief Account of a Short Odyssey

In terms of self-identity, I think of myself as a student of stratification, not an educationist. I also happen to work out of a quantitative/empirical tradition, but this is incidental to the "fundamental" character of my scholarship. I suppose a third confession is in order at this point: Despite having spent the better part of my research career studying schools and their workings, I never have had much of an intrinsic interest in educational issues. Schools are prominent in my work as arenas in which stratification processes play themselves out. This is my "hook" into education research. It has led me to study school effects and effectiveness, various facets of school organization, and the social-psychology of school attainment processes. Let me review how one gets from here to there.

"Who gets what, and why?" A catchy phrase sticks, and I remember learning this one as a graduate student from Gerhard Lenski. It captures the essential problematic of social stratification as a field of study, and is both profound and rich with possibilities. Stratification presents itself in many guises. One is stability over time in the structure or patterning of social inequalities. Occupational opportunity has long been viewed by students of stratification as a particularly sensitive measure of the openness or rigidity of a society's stratification system, and for this reason studies of intergenerational and intragenerational occupational mobility early on came to dominate

the agenda of empirically oriented stratification research (for an early treatment of these issues, see Lipset and Bendix, 1959).

A typical study of intergenerational mobility would take data on the current occupations of a sample of adult workers and map it onto a parallel classification of the occupations of these workers' fathers, referenced to some common time period in their childhood (typically obtained retrospectively from the respondents; e.g., "what was your father's main occupation when you were growing up?", or some such query). The researchers then could look to see what fraction was working in the same category as their fathers (often construed as a measure of "occupational inheritance"), what fraction had occupations above the standing of their fathers (a measure of upward mobility, assuming that the occupations were ranked in some way), and what fraction had occupations below the standing of their fathers (a measure of downward mobility). Similar questions could be posed of data that characterized occupational progressions within individual workers' careers, so-called intragenerational mobility.

To be honest, I have always found such enterprise exceedingly boring. After all, you can only stare at two data points with enthusiasm for so long, and after that story's told then what? Nevertheless, this was, and remains, an important genre of research. In particular, it has demonstrated a high degree of orderliness in mobility patterns, especially in mobility across generations. Recast in terms of the "who gets what, and why" question, this means that individuals' own career prospects are limited, for better or for worse, by the characteristics of the families into which they happen to be born, and in particular by the occupational standing of their parents.

This helps us understand "who" and "what" but leaves open "how" and "why." These questions always have intrigued me, but they are not the stuff of traditional mobility studies. The floodgates opened, though, with the publication of Blau and Duncan's *The American Occupational Structure* in 1967.

Blau and Duncan began with a conventional analysis of occupational mobility patterns. This, though, was preliminary to a critical demonstration: that most of the movement between occupational categories in the cross-classification of origins against destinations could be captured as movement up and down a status *continuum*, along which occupations could be reliably and validly ranked.[1] Instead of looking to patterns of occupational mobility as the window on stratification, the degree of association between origin and destination measures of occupational status would tell much the same story. The trick was in substituting quantitative measures of origin and destination (i.e., occupational status) for qualitative measures (i.e., occupational category), thereby opening the door for embedding these relationships in larger, multivariate systems.

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1 This was the metric of the Socioeconomic Index, SEI, which Duncan had developed some years earlier.

Blau and Duncan's "basic model of the process of stratification," reproduced in Figure 1, was the point of departure for what has come to be known as "status attainment" studies. Two objectives are encompassed by this framework: first, to describe quantitatively how *various* aspects of family background, not just father's occupation, constrain adult attainments; and, second, to learn how it is that these constraints actually work. The first agenda item is reflected in the inclusion of father's education as a complement to father's occupation in expanding coverage of family background resources. The second, and more important for my purposes, is reflected in the inclusion of education level as a predictor and in the distinction between first job status (after finishing school) and present job status.

The fact that these are portrayed as intervening between family background and present occupation is quite important, as is the pattern of arrows linking prior variables to subsequent ones. The arrows embody the model's assumption regarding possible influence flows, and the statistical technique used to evaluate the model (i.e., path analysis) yields coefficient estimates for each path or link. The inclusion of possible intervening, or mediating, variables represents an attempt to "interpret," in Lazarsfeld's sense of the term (see, for example, Lazarsfeld, 1955), the relationship between origins and destinations by identifying the more proximate mechanisms that maintain it. This is one mode of explanation; that is, a way of addressing questions of the "how" and "why" variety.

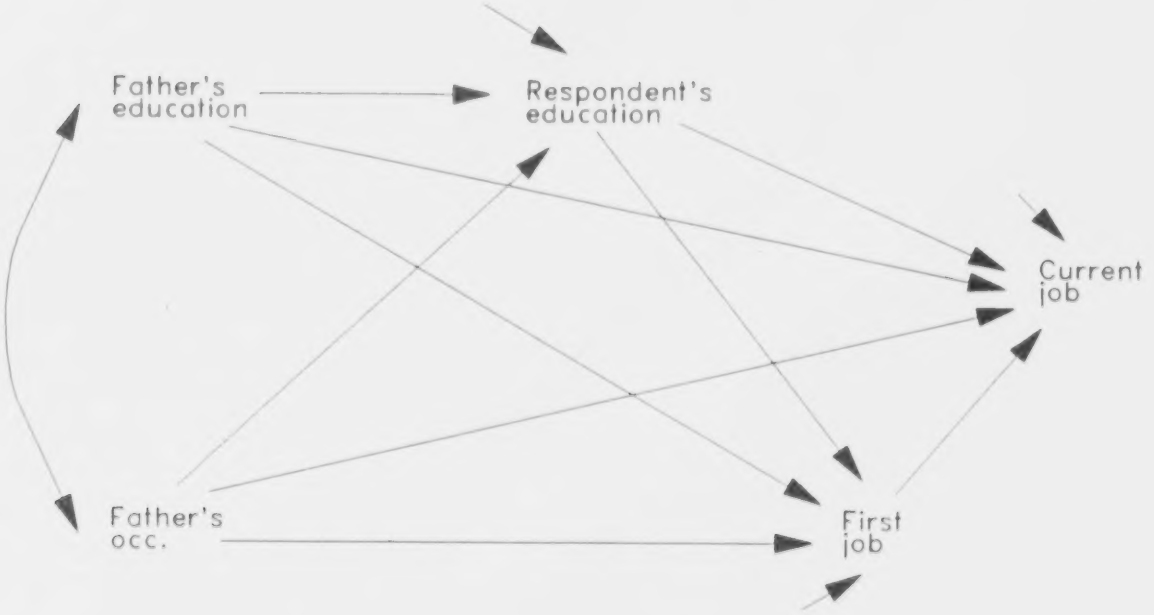

Figure 1. The Blau-Duncan basic model of the process of stratification.

To play out the Blau-Duncan example, we know that children born into high-status households are much more likely as adults to find themselves in high-status occupations than are children born into low-status households. But what is the basis for this advantage; how does it come about? Part of the answer has to do with the high level of stability in career lines from first job to present, and the fact that youngsters from high-status families tend to get better first jobs. But then we would want to know why they have better job placements initially, and results from the Blau-Duncan "basic model" tell a clear, and I think convincing, story: To a considerable extent, it is because they tend to go farther through school than youngsters from low-status families.

With some 20-plus years of hindsight, this hardly seems a startling revelation, but the Blau-Duncan model was the first to articulate the problem in these terms *and* to perform an analysis that would quantify precisely the extent to which schooling serves as a conduit for family-based patterns of advantage and disadvantage. Blau and Duncan's study identified educational level as a critically important link. It accounted for virtually all of the influence of father's education on son's occupational level and for most of the influence of father's occupation. Additionally, educational level itself was found to have important effects on occupational status over and above those involving the transmission of background advantages and disadvantages.

The Blau-Duncan "basic model" was understood even then to be but a point of departure. In fact, it was quickly superseded in Duncan's own work, and subsequently has been elaborated and refined in many ways (see, for example, Duncan, 1968 and 1969; and Duncan, Featherman, and Duncan, 1972). But as a point of departure for those of us interested in the persistence of inequality across generations, its "next step" implications were obvious: we needed to learn more, much more, about the sources of educational inequality, for this is the main foundation of the link between status origins and status destinations. Why do some children stay in school longer than others; and while there, why are some more successful than others? In light of insights from the Blau-Duncan analysis, such questions assumed significance that went beyond their immediate context, and thus did many students of stratification find themselves preoccupied with the workings of schools.

Among the first to take up the challenge were researchers from the University of Wisconsin, under the leadership of Bill Sewell (e.g., Sewell, Haller, and Portes, 1969; Sewell, Haller, and Ohlendorf, 1970; Sewell and Hauser, 1980; Hauser, Tsai, and Sewell, 1983). Advancing a social-psychological perspective, the Wisconsin researchers looked to differences in family socialization processes for clues to middle-class youngsters' educational advantages. They reasoned that family values, resources, and experiences in higher-status households, as against lower-status ones, would be better aligned with the achievement-oriented values and behaviors of the typical school environment. Youngsters who are raised to think and act in ways expected of them in the institutional environment of the school ought to be more successful there, and this, they suggested, might help explain the linkage documented by Blau and Duncan between family background and

educational attainment. Implied in this line of reasoning is a developmental chain that starts with social and economic conditions of the family, moves through the kinds socialization experiences the child encounters, impacts on the child's attitudes, habits, and values, and results, finally, in attainment and achievement patterns that are differentiated along socioeconomic lines.

An operational counterpart of this general imagery—the so-called Wisconsin School Process model, so designated because of its focus on interpersonal influences in the schooling process—was introduced by Sewell and his colleagues in a 1969 publication, and this set the stage for more than two decades of extraordinary productivity by the original investigators, along with important contributions by others who were inspired by their example (e.g., Alexander, Eckland, and Griffin, 1975; Duncan, Featherman, and Duncan, 1972; Haller and Portes, 1973; Jencks, Crouse, and Mueser, 1983; Otto and Haller, 1979; useful overviews are provided by Bielby, 1981, and by Campbell, 1983).

The Wisconsin framework postulates that the attainment advantages associated with favorable home circumstances and high cognitive skill levels come about in part because these qualities encourage good performance in school; in part because the parents and teachers of such children are more likely to encourage them to go to college (which presumably encourages them to apply themselves to their studies and to persist in school); in part because their friends, who serve as role models, are more likely to intend to go to college; and in part because all of these considerations orient youngsters toward high educational and occupational goals, which also help channel energies toward educationally constructive ends. The analysis that corresponds to this series of statements quantifies all the permitted paths of influence, as well as the routing of effects of prior variables through intervening or intermediate mechanisms. The result is an empirically mature representation of the conceptualization.

The Wisconsin researchers broke exciting new ground in showing how to organize conceptually a complex set of ideas linking social structure, socialization processes, personal development and educational attainment and in showing how to evaluate that conceptualization empirically with a methodology that respected both the whole and the details. This was no small accomplishment, and it established a new style of research on weighty educational concerns that continues some 20 years later to yield valuable insight. What is most important about this breakthrough in terms of my concern with basic research is its borrowing of propositions from the field of social-psychology to flesh out the sources of socially patterned educational inequalities.

This social-psychological, actor-oriented perspective dominated sociological studies of education for many years. From this vantage point, though, the school and the classroom are little more than backdrops, settings within which socialization experiences come together and students play out their inclinations. The idea is that the social world expresses itself via its impact on students' personal development; thereafter, their fate (education-wise, at least) hinges largely on the resources they bring to bear on the competition—their interests, skills, ambition, and work habits.

No doubt there is considerable truth to this characterization of what makes for success in school, but just as surely it is not the entire story. Neglected in this perspective are organizational features of schools that can shape students' experiences, and even govern their outcomes, quite apart from anything having to do with their interests, competencies, and the like. Schools and school districts control and deploy resources, they adopt a curriculum and put into place practices for its implementation, agents of the bureaucracy occupy "gatekeeper" roles vis-a-vis students and their actions can open or foreclose opportunities, and the quality and character of the organizational setting (e.g., context and climate) establish the kind of environment for learning that prevails. Kerckhoff (1976) subsumes many of these themes under what he refers to as an "allocation" perspective on school attainment processes. The "allocation" imagery is intended to connote a more active role for schools as organizations—doing things to students that somehow alters their trajectories—as distinct from the more passive imagery of schools as either settings in which interactions take place or as conduits through which talent flows.

Painted in broad strokes, this is the historic backdrop of research on stratification and the schools. Curiosity about the persistence of social inequality across generations is the point of departure, and differences in school success were identified early on as central to the problem. The challenge, then, became to understand how these differences arise, and it is toward this end that insights from social-psychology and from the perspective of organizational structure and functioning have been brought into the domain of education studies. The next step is to consider what can be learned from research of this sort. Some possibilities are illustrated in the following section, organized around the themes of "social inequality," "social-psychology of the schooling process," and "school organization."

Some Fruits of Fundamental Research: An Illustrative, Highly Selective Overview

Social Inequality

The persistence of socially structured inequalities in mature industrial and postindustrial societies such as ours long has intrigued students of stratification as something of an anomaly. The engine that drives the modern social order is human talent—the skills, cognitive and otherwise, creativity, and ambition of the citizenry. Since an expanding pie serves everyone's interest, society ought to strive to identify and nurture its "human resources" without regard to traditional biases or barriers. Where the cult of efficiency prevails, it's what you can do that counts, not who you happen to be. This construction of the modern order as "meritocracy" anticipates that "effort" and "ability" will be the tickets to success, and obstacles not grounded in distinctions of merit are thought to be anachronisms that eventually will yield to the dynamic of modernization. Sounds good, eh? Also, unfortunately, rather fanciful.

One brake on this meritocratic dynamic, perhaps even the major one, is the persistence of the traditional family form, and the parochial interests it embodies. Most parents want to provide well for their children and to see them do well, even if they're not the brightest, the most imaginative, or the most energetic of souls. And, of course, some families are better situated than others to do so.

Therein resides the tension in "family-based meritocracy," as the "particularistic" interests of parents in taking care of one's own come into conflict with the "universalistic" interests of society in seeing that talent finds its own level. The early literature, as reviewed above, focused on liabilities having to do with socioeconomic background, as it is the family's position in the social order that largely determines what resources it can bring to bear on this competition for advantage. However, as interesting and important as this might be from a theoretical point of view, it really doesn't get at what troubles people most. Rather, from a social problems perspective, it is racial discrimination, and more recently disadvantages revolving around gender, that most weigh on the collective conscience, and the research agenda quickly broadened to accommodate these concerns.

Much of this research focuses on disadvantages in the labor market (e.g., the troublesome wage gap between men and women and between blacks and whites) and is not of immediate relevance. But the fact that minority and disadvantaged groups tend not to go as far through school as white males, and as a consequence enter the labor market with less useful educational credentials, contributes to their economic "shortfall." It is for this reason that research into the reasons for unequal *educational* attainment can help us understand unequal *economic* attainment.[2]

Studies of educational disadvantage have focused on two dimensions of the problem, persistence and performance. The former pertains to progress through the educational system, as reflected in such things as years of school completed or highest degree obtained. This often is referred to as level of educational attainment, and is the focus of "attainment" studies in the sociological tradition. Performance, on the other hand, refers to scholastic accomplishment or skill

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2 Several important qualifications are in order here, as educational differences are much less implicated in the income or earnings gap separating whites from blacks and males from females than most people probably think. For one thing, education is only moderately strong as a predictor of income and earnings differences altogether.. For representative samples of mature adult workers, the zero order correlation between years of schooling completed and annual earnings/income hovers about .40 (see Jencks et al., 1979, for a compilation of relevant data). Also, the trend in educational levels for blacks and whites and males and females has been toward convergence. The average "gap" at present is small in both instances, and therefore cannot account for much of the gap in economic indicators. Jencks et al. (1979, p. 203), for example, estimate that if the nonwhite educational distribution were to come into alignment with the white distribution and other determinants of economic well-being remained unchanged, the white-nonwhite earnings difference would shrink by about 25-30 percent. While educational inequalities certainly are implicated in economic inequalities, they are hardly the entire story, and this deserves recognition. Closing the educational gap would be expected to make an important dent in persistent economic disparities, but it is not the "great equalizer" that many would like to believe.

level. It is reflected in such things as test scores and grade performance. Historically, disadvantaged groups have not fared well in terms of either persistence or performance, and this has been cause for considerable concern.

As a point of departure, fundamental research can document the extent of such inequalities, and identify where they are most pronounced (e.g., is the attainment "gap" between blacks and whites due mainly to differences in high school dropout rates, differences in college attendance rates, or differences in college persistence?). Descriptive detail of this sort is extremely important, as it is through such monitoring that we come to understand the magnitude and dimensions of the problem. But this is basically a matter of plotting trends, and a fundamental research perspective makes no distinctive contribution. However, by embedding description in an analytic framework an interpretive context is established that can help draw out implications.

A common approach is to distinguish differences between groups that might be grounded in "merit" considerations—differences in skill levels, ambition, etc.—from those that lack any such basis. Inequalities of the latter variety are offensive by any reasonable standard. They reveal the extent to which race, or gender, or family background per se is disadvantaging. Disparities of the first variety, though, are less clear in the valuations that might attach to them, as they are based on considerations that otherwise might be deemed quite acceptable, even commendable—in an achievement-oriented meritocracy, success "ought" to accrue to those with the requisite talent and drive. If one accepts this logic (and not everyone does), then the problems implied by merit-based, patterned inequality are of very different character from those involving, say, discrimination in the marketplace or in the workings of schools. Instead, they imply "resource" gaps, and possibly too a very different agenda for corrective intervention.

Regarding educational attainment, the research record reveals large and persistent differences having to do with family socioeconomic background, smaller differences involving race/ethnicity, and, especially in recent years, only tiny differences involving gender (see, for example, Alexander, Riordan, Fennessey, and Pallas, 1982). The criterion here is years of school or highest degree completed, and the details well could differ for other considerations, such as type of college attended or college major selected. But we have to start someplace and how far one goes through the educational system undoubtedly is the most appropriate point of departure. Certainly insofar as career considerations are concerned (e.g., such things as earnings and occupational placement), level of schooling is far more consequential than any other broadly applicable educational distinction. This, of course, is why dropout prevention programs and policies to broaden postsecondary access have commanded so much attention.

Not only are socioeconomic factors much more important than other ascriptive lines of demarcation when it comes to the patterning of attainment differentials, but "merit" considerations further confound the conventional wisdom. Research has shown, for example, that the black-white difference in educational level can be accounted for entirely by correlated differences involving

socioeconomic background and measured academic ability. And pretty much the same pattern holds at various school progression "benchmarks": high school dropout; the transition from high school to college given high school graduation; and completion of an undergraduate baccalaureate degree given college attendance. In each instance, the black-white gap is small to begin with, it closes entirely when black-white differences in socioeconomic background are adjusted for, and it *reverses* when black-white differences in cognitive skill levels and school grades are adjusted for. The situation is very much the same in comparing the experiences of Hispanic youngsters against whites, except that the original disparities generally are larger and a small difference in college attendance probabilities remains even after these statistical adjustments are implemented (these trends are documented in Alexander, Riordan, Fennessey, and Pallas, 1982; Alexander, Holupka, and Pallas, 1987; Alexander, Pallas, and Holupka, 1987; Pallas, 1986; Thomas, Alexander, and Eckland, 1979; Rumberger, 1983; Wagenaar, 1987).

That these racial gaps close when SES differences by race are taken into account leaves open whether the original differentials reflect racial or socioeconomic disadvantage. A current theory (Wilson, 1978) suggests that racial/ethnic liabilities should recede in favor of class differences under prevailing economic conditions, and this pattern is consistent with that expectation. The finding regarding academic controls indicates that when blacks and whites with similar test scores and high school grades are compared, blacks actually are somewhat *more likely* than whites to finish high school, to go to college, and to complete a college degree. We know that black test scores and academic performance generally fall below corresponding white test averages and grades, and this, we now realize, also takes its toll in terms of school continuation and progression.

What all this actually means is still very much open. One possibility is that the customary measures of competency underestimate minority youngsters' abilities, but I know of no compelling evidence to this effect (e.g., Jensen, 1980). Another is that understanding the reasons for minority youngsters' academic underachievement assumes even greater significance, for it is the key to other weighty educational disadvantages. This, I should add, is not simply a matter of "blaming the victim," although it certainly could be cast in those terms. Rather, I think of it more as discovering the right questions to be asking. Whichever the case, it at least is apparent that simple descriptive data on the patterning of educational inequalities leave many important aspects of the story untold.

Inequalities associated with socioeconomic background not only are the largest of the "ascribed" burdens, but they also are the most persistent and the most pervasive. Children born to low SES households are disadvantaged at every juncture. And even though they too are prone to perform poorly in school, this does not account for their other difficulties to nearly the same extent it does racial-ethnic differences. Differences revolving around SES level persist at every benchmark after adjusting for test scores and grades, and, in fact, even after adjusting for several other important assets in the schooling process. For example, when considering years of schooling completed, *only* about half the SES "gap" can be accounted for by the fact that youngsters from advantaged

households tend to test better, get higher grades, have higher aspirations, are more likely to take an academic program of study in high school, are more likely to have friends who plan to go to college, and are more likely to be encouraged to go to college by parents and teachers (see, for example, Alexander and Eckland, 1980).

I say here "only" to emphasize the enduring reality of socioeconomic stratification, but one person's "half full" is another's "half empty," and these same results indicate that, to a considerable extent, low SES youngsters terminate their schooling before their higher SES peers because they don't test as well, because they get poorer grades, because they aren't as likely to take a college preparatory program in high school, etc. Hence, we see that high SES youngsters reap the benefits of being better students, of being more achievement oriented, of more "constructive" socialization experiences revolving around their network of intimates (i.e., parents, teachers, and friends) and of academic preparedness through an appropriate program of study.

Included here are themes involving the social-psychology of socialization processes and mechanisms of organizational facilitation, to which we shall return shortly. What is important in the present context, though, is how their inclusion in research on patterns of educational inequality deepens our understanding of what is at issue.

The picture with respect to gender differences is rather different, as the gender gap in educational level is small altogether and very little of it can be attributed to these mechanisms (in large measure because gender differences on them are either trivial or tend to favor girls, as in grade performance—see Alexander and Eckland, 1974). Rather, other research suggests that family role transitions (e.g., marriage and childbearing/rearing) interfere more with women's school continuation than with men's (Alexander and Reilly, 1981; Bacon, 1974; Marini, 1978; Rumberger, 1983). These insights from fundamental research teach us that not all inequalities are alike. In doing so, they also tell us that suspected explanations ought to be tested rather than taken for granted, at least, that is, if one really wants to know.

As the above remarks indicate, questions pertaining to patterns of progress through the education system have received considerable research scrutiny. There also are quite substantial literatures having to do with achievement or performance patterns, again with an eye toward documenting and understanding inequalities. Longstanding concern over declining test scores and the focus of the recent education reform movement on cognitive performance attest to the perceived importance of these matters. (These concerns are well represented in the document that triggered the recent flurry of reform initiatives, *A Nation at Risk* (National Commission on Excellence in Education, 1983.) And in light of what was said above regarding the contribution of performance differences to attainment differences, these issues assume significance beyond their intrinsic importance.

One of the early attempts at a systematic accounting of performance differentials on a broad scale is the well-known Coleman, or Equality of Educational Opportunity (EEO) Report (Coleman, et al., 1966), commissioned by Congress in the Civil Rights Act of 1964. The EEO design entailed a massive cross-sectional survey and testing program, covering hundreds of thousands of students and more than a thousand schools at all educational levels.

Although the EEO Report afforded only a snapshot of the moment, to say that the picture it drew was sobering would be an understatement. Focusing on data from the project's test of verbal performance,[3] the Report demonstrated that minority performance lagged well behind white at the earliest grade level it could be reliably assessed (the third), and that the gap, if anything, grew over the years.[4] Certainly there is no indication of convergence in the EEO data, which one would expect to see if the schools were having success in remediating the academic difficulties that many minority youngsters apparently experience in the early grades. While there are encouraging indications that the black-white testing gap has closed a bit in recent years (see, for example, Burton and Jones, 1982; Jones, Burton, and Davenport, 1984), it still remains large and worrisome.

The EEO survey also documented large, across-the-board performance differences by SES level. It further established that while SES background and race/ethnicity overlap to some degree (meaning minority communities also tend to be socioeconomically disadvantaged), each is related to cognitive performance independently of the other. What this means is that race differences in school performance have something to do with the minority experience, and are not simply fallout from socioeconomic factors. This makes the character of the performance disparity possibly very different from the attainment disparity, as there is no need to invoke race-specific explanations for the latter.

The EEO Report's description of performance patterns was preliminary to a wide-ranging search for school characteristics that might help us understand why some students, and some groups, do better than others. The expectation was that the schools attended by minorities would be found grossly inferior to those attended by whites, and that much minority underachievement would be attributed to inadequacies in the schools. To practically everyone's surprise, though, the Report's evidence failed miserably to sustain the then-conventional wisdom: first, the schools attended by minority students were not especially lacking on at least the gross measures of quality that were available in the EEO data (e.g., facilities, resources, staffing, program offerings, etc.); second, and more generally, the numerous school characteristics covered in the Report's analysis had very little bearing on students' test performance, for any group or at any level of schooling!

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3 This, as it turns out, is not especially restrictive, as all the tests in the EEO battery told essentially the same story.

4 Whether the differential actually was larger in the 12th grade than in the third is difficult to determine because of various design problems, one of which is the exclusion of high school dropouts from the 12th grade survey.

It had been expected that the Report would lay out a blueprint for school improvement by identifying those things we needed more of, for which checks could be written: books, laboratories, accelerated curricula, more teachers with Master's degrees, or whatever. (The then-prevailing climate of opinion is covered nicely in Mosteller and Moynihan, 1972.) However, apart from a hint that minority students perform a bit better in schools with a large enrollment of high SES students,[5] the Report implied that one would have to look elsewhere for the solution. But where?

The EEO Report occasioned considerable reflection. Many simply refused to accept its message, and there was ample ammunition for those inclined to fault the Report, and perhaps even dismiss it, on methodological grounds. But its general conclusions have stood up surprisingly well (see various analyses in Mosteller and Moynihan (eds.), 1972; Hauser, 1969 and 1971, Jencks et al., 1972), and those who accepted at least its broad outlines, yet remained convinced that schooling does matter for cognitive development, struck out in many different directions. Some of these will be reviewed later, in the section on school organization.

The experience of the EEO Report, in the very failure of its agenda, illustrates another important contribution of fundamental research on educational questions: its potential for "debunking." The social sciences often are faulted as simply confirming the obvious, but some of their more valuable contributions have been quite counterintuitive. These experiences remind us that impressions can indeed be misleading, and that sometimes what seems simple and obvious upon closer scrutiny turns out to be both complex and subtle. This, I think, is very much the case with our understanding of school effectiveness.

Despite the many uncertainties that surround the EEO Report's treatment of these issues, the Report certainly established that whatever complicates matters for minority and disadvantaged students in school must take hold very early. This is one reason that Doris Entwisle and I chose to concentrate on the transition into first grade in our ongoing study of young children's cognitive and affective development, which we have designated the Beginning School Study (BSS). Role transitions in general are stressful, and since schools are heavily infused with aspects of middle-class culture, we reasoned that this settling-in period might be especially difficult for minority and disadvantaged youngsters. We determined to look forward from the beginning of first grade to see if we could observe the onset of these problems and trace out their sources and their repercussions.

The BSS randomly sampled about 800 beginning first graders from 20 schools in the fall of 1982 and has been monitoring their progress regularly since then. In the fall of first grade, the black and white youngsters in our sample had very similar averages on verbal and quantitative subtests of the California Achievement Test (CAT) battery. However, by the end of first grade, the black

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5 This finding, incidentally, was the basis of the Report's support for school desegregation initiatives.

average lagged behind the white in both domains, and blacks received lower report card marks in every quarter, including the first. The test score spread increased a bit through the end of second grade (which is as far as our analyses extend to this point), but not dramatically so, and what we have seen thus far is basically the persistence of achievement differences that arose during the initial period of adjustment in first grade (see Entwisle and Alexander, In Press(b)).

Our analyses identify numerous predictors of cognitive growth over first grade, and many more than in the second grade. This suggests more fluidity during the unsettled, and unsettling, period of adjustment, as well as perhaps a window of opportunity for constructive intervention. Yet most of these predictors involve family and personal qualities not under the school's control, and there are fewer significant predictors of growth among blacks than among whites. Correspondingly, and importantly we think, fall to spring CAT score correlations, and first quarter to fourth quarter grade performance correlations, are higher among blacks than whites, as are the effects of marks from early in the year (i. e., first quarter marks) on end of year test performance.

The combination of higher over-time stability and fewer nonperformance predictors of spring CAT performance among blacks describes more "closed" or more "constrained" achievement trajectories for them, as compared to the situation for whites (Alexander and Entwisle, 1988; Entwisle and Alexander, 1988). That is, in the case of minority youngsters there appear to be fewer school-based sources of leverage with which to make inroads on the problem. However, one specific source of difficulty identified in our work is teacher-pupil "mismatch." Teachers who themselves came from higher-status family backgrounds were less likely to perceive low SES and minority children in a favorable light. They evaluated such children as being less mature and held lower performance expectations for them than for students of similar ability whose SES characteristics more nearly matched their own. Higher SES teachers in such situations also reported less favorable assessments of the school climate. These indicators of social distance and teacher disaffection resulted in depressed marks and test scores, and were especially harmful to the performance of low SES and minority youngsters (Alexander, Entwisle, and Thompson, 1987).

This is one respect in which the social relations of schooling are found to work to the disadvantage of disadvantaged students. On the other hand, our analysis of seasonal variations in cognitive trends (comparing summer growth with winter growth) indicates that the experience of schooling itself is strongly beneficial for such youngsters, with the exception of the lowest SES blacks. When school is not in session, the growth curve for these "at-risk" children is relatively flat, while that for more advantaged youth is quite steep. During the school year, however, the pattern is reversed, and disadvantaged youth make up much (but not all) of the ground lost to their more favorably situated peers during the summer. The latter youngsters, it seems, are not as dependent on the schools for their intellectual progress, which is plausible in light of their more plentiful home and community resources. Schooling helps fill this gap for disadvantaged youth, and in this sense constitutes an important "compensatory" intervention (Entwisle and Alexander, In Press(a)). This

is a far cry from the "schools don't make a difference" conclusion that many mistakenly read into the original EEO Report (e.g., Hodgson, 1973). As presently constituted, schools apparently are not sufficiently potent to overcome all the difficulties flowing from a disadvantaged background, but clearly they play a highly constructive and important role, and it is through fundamental research that this lesson is learned.

The situation with respect to gender differences in performance is a bit more complicated, in that the pattern varies with age and is dissimilar across criteria. Girls tend to get higher grades than boys, even in domains such as math where their objectively assessed skills sometimes lag behind. In the early grades this often is attributed to girls' maturity advantage over boys, and throughout schooling girls' comportment generally is more agreeable than boys'. In the BSS data (Alexander and Entwisle, 1988) boys' and girls' levels of personal maturity in first and second grade are quite similar, at least as evaluated by teachers, yet girls receive higher conduct marks from the very start. This suggests that conformity rather than maturity may be more important to girls' school success. That is, girls are better behaved in school than boys, and this shows up in better grades, which, after all, are social constructions (see Maccoby and Jacklin, 1974 and Hyde and Linn, 1986 for overviews).

Gender advantages in skill areas differ across domains, and there is some evidence that these are diminishing (Feingold, 1988). The one that has attracted the most interest is girls' quantitative shortfall relative to boys'. This first shows up in early adolescence when reasoning abilities become prominent in assessment devices. Tests for younger children tend to load more strongly on computational skills, while those designed for older youngsters place greater emphasis on quantitative reasoning ability. Recent evidence suggests that these two kinds of skills develop independently and rather differently (Entwisle and Alexander, in press). This makes it hard to interpret longitudinal data on composite math scores.

Some researchers attribute the emergence of boys' advantage to differences in quantitative reasoning ability, postulating a possible biological basis for it (e.g., Benbow and Stanley, 1980). There is ample reason to suspect important social components though. Entwisle and Baker (1983; see also, Baker and Entwisle, 1987), for example, found that parents' gender-based ideas about their children's abilities affected quantitative school performance in the early grades, and my own work (Pallas and Alexander, 1983) indicates that about 60 percent of the difference in boys' and girls' performance on the SAT-M *that emerges during high school* can be accounted for by differences in boys' and girls' math and science course-taking patterns and course marks. This is referred to in the literature as the "differential coursework hypothesis," and it seems to be very much implicated in the quantitative skill "gender gap" (see also Fennema and Sherman, 1977; Steel and Wise, 1979). Here again, while documenting differences is a useful service, the greater challenge is to understand them, and description in an interpretive context can offer useful clues.

Social-Psychology of the Schooling Process

To say that schooling takes place in a social context may border on trite, but it also is rich with import. School attainments and achievements are, after all, samplings of human behavior, and they ought to be governed by the same principles that apply to other domains of performance. But to be useful, general principles must be contextualized. So the roles being enacted are "student," "parent," and "teacher," the most salient motivations and self-understandings pertain to things academic, the competencies at issue are those relevant to academic tasks, and group process takes place in the classroom and the home. As indicated, much research has focused on social and interpersonal aspects of the student as academic performer. He or she must do the work, but the resources brought to bear on the task have to come from somewhere, and the social-psychological underpinnings of school achievement/attainment processes have commanded special attention.

Many of these themes are embodied in the so-called Wisconsin framework described above, and some of the specific findings that bear on them already have been touched on in my comments on schools and inequality. The Wisconsin approach takes as its point of departure the student as goal-directed actor. This is reflected in the central role accorded educational expectations and occupational aspirations in its conceptualization. That these are important determinants of educational and occupational attainments seems to sustain the wisdom of this emphasis. We know that youngsters from advantaged family circumstances are more likely to aspire to high goals, and the Wisconsin framework looks to the students' relations with various "significant others" and to self-reflection as the more immediate forces shaping these goals (see, for example, Woelfel and Haller, 1971). Parents and teachers offer encouragement for college that is conditional on both family background (youngsters from high SES families are more likely to receive such encouragement) and on demonstrated ability, as revealed through school grades. In fact, parents' social-psychological support for school success seems to be more important than material and economic aspects of family well-being in studies that try to separate the two (Alexander and Eckland, 1980; Sewell and Hauser, 1980).

Teachers too can be an important source of either encouragement or discouragement, but this is a social, as opposed to professional, dynamic. There is little indication from what is now a quite considerable literature that the standard measures of teacher "quality"—years of experience, possession of an advanced degree, type of undergraduate college—matter much at all (see Levin, 1980, for relevant commentary). Teachers' attitudes and supportiveness, on the other hand, can make a considerable difference (Alexander, Entwisle, and Thompson, 1987; Smith, 1972), indicating from yet another vantage point how social-psychological factors intersect school achievement processes.

Peers also help goals take form. The Wisconsin perspective emphasizes one-on-one diffusion of values, with friends influencing friends by way of example. Other approaches plac

greater emphasis on student cultures and subcultures, and how participation in these can absorb and (re-)direct youths' energies (e.g., Coleman, 1961). The two perspectives are complementary.

And, of course, the object of all this attention, the youngsters themselves, are not simply passive recipients of "input" from their social surroundings—socialization sponges, if you will. Rather, they must attend to these various clues and cues, and filter them through the lens of their own experience before they have an impact on the course of personal development. Research shows that youngsters differ greatly in the way they process feedback from the environment—some do so much more veridically than others—and that some of these differences are socially patterned. Youngsters from middle-class families, for example, apparently attend more to their own performance in framing academic goals for themselves than do their lower-status peers (Entwisle and Hayduk, 1978).

Similar differences also apply to parents as they form expectations for their children's performance (Entwisle and Hayduk, 1978; 1982). We know, for example, that blue-collar parents do not take account of their children's past performance to nearly the same extent as white-collar parents in framing their expectations. Their encouragement in this sense is offered unconditionally, and often it will be at odds with feedback that the child gets from other sources. When this happens, parents' credibility with respect to things academic suffers, and with it their ability to play a constructive role in helping shape their children's academic development. This literature, then, reveals important differences across SES lines in parents' efficacy as agents of academic socialization.

And, finally, the individual student also is a presence as one of the actors on the scene, and how others react to him or her no doubt can have considerable bearing on what is received in return. One literature to pursue this idea is the research on teacher expectancies, as exemplified in the well-known Pygmalion experiments initiated in the mid-sixties by Rosenthal and Jacobson (1968). Before turning his attention to educational issues, Rosenthal had done extensive research on the social-psychology of the psychological experiment, exploring the possibility that researchers sometimes behave in ways that bias results in favor of whatever hypothesis is being evaluated, and that this often is entirely inadvertent and unconscious (see Rosenthal, 1966). Since Rosenthal's experimental situations were structured so as to be neutral with respect to the study's purported hypothesis, when supportive results came in, as they often did, they actually gave witness to the potency of expectations as a governor of behavior.

This was a classic case of the self-fulfilling prophecy, and when transported into the classroom it focused attention on teachers' expectations of students' potential as a stimulus to their cognitive growth. Rosenthal and Jacobson's first experiment seemed to show that when teachers were led to expect, via false information, that some students would be "intellectual bloomers," those students tended to make larger than expected strides in terms of standardized test score gains. This was taken as demonstrating the power of ideas, and turning it on its head seemed to offer a plausible

accounting of the academic problems experienced by many minority and disadvantaged children: because of racial or class biases, teachers expect less of them, and as a consequence behave in ways that fail to draw out their potential. To make matters worse, this attitude is contagious. Other pupils quickly pick up the teacher's "definition of the situation," and eventually even the objects of their scorn come to see themselves as failures, thereby seeming to validate the teacher's initial preconceptions. This is the morality play in Rist's Factory for Failure (1973). It is a vicious cycle, with the children as victims.

This characterization of the sources of disadvantaged students' academic difficulties attracted a considerable following, and no doubt many remain true believers. Unfortunately, it seems to have oversold itself. The original Pygmalion experiments have proven themselves highly resistant to replication despite many attempts, and in general there is little support for Rist's idea that mean-spirited teachers are the source of all these difficulties (see Wineberg, 1987, on both counts). At the same time, it is beyond dispute that real people in the real world, teachers and pupils, do form impressions out of their contact with one another. Typically these are grounded in a reasonably accurate reading of cues, and they tend to reinforce, not deflect, patterns that have already been laid down (see Brophy, 1983; Dusek, 1975), but research has identified some situations that seem to be especially susceptible to self-fulfilling prophecy-like interpersonal processes. As we have seen in the BSS data (Alexander, Entwisle, and Thompson, 1987) certain conditions of "mismatch" between teacher and pupil social backgrounds set up interference that complicates matters for minority and disadvantaged youngsters. Poor teacher morale and inappropriately low expectations can, and do, damp achievements, but the early literature lead us to believe such problems were pervasive, and this simply isn't so. Recent studies have clarified the picture by revealing the conditions under which pupil-teacher relations gravitate toward this degenerate form, and in doing so the fundamental research approach has proven itself healthily self-correcting.

This material reminds us of the importance of the social and interpersonal context in which learning takes place. Achievement is not simply the unfolding of extant skills or of personal dispositions, as important as these are. Rather, it is drawn out by a complex of forces involving numerous personal characteristics, conditions in the family and conditions in the school. There is considerable risk, then, in studying schools in isolation from the other institutional settings that intersect their functioning. The search for interventions via "alterable" features of classrooms or schools may be especially prone to such premature narrowing of the field. The "big picture" needs to be understood, even if it can't be changed, because this tells us where the things that can be changed fit in. The broader social context and the more immediate interpersonal context both play an important part in students' academic development, and the social-psychological approach helps us see these connections more clearly.

School Organization

The Equality of Educational Opportunity Report, introduced above, was a singular event, both for what it revealed about the workings of the educational system (generally disturbing) and as a stimulus to further study. In fact, that the Report encouraged a healthy reconsideration of what made for effective or successful schools no doubt is one of its more lasting contributions. Several promising lines of research followed, along with an occasional false start.[6]

One of the more intriguing conclusions from the EEO Report was that the kinds of students with whom one attends school can make a difference, in this case for black youngsters going to schools with large enrollments of high SES students. Black youngsters seemed to perform better in such situations. The Report's conclusion that characteristics of the student body can influence the character and quality of the school experience ties in with other interesting literatures, some of which predate the EEO effort (see, for example, Wilson, 1959) and all of which invoke general propositions regarding either the social-psychology of schooling or organizational functioning to try to comprehend the meaning of such findings.

This is where the basic research perspective enters the picture. Reference group processes are prominent in thinking about how the student body can set a distinctive tone. There are two major thrusts here. One is the idea that students tend to acquire or internalize the standards they observe about them as a sort of looming presence—the school climate or ethos notion (McDill and Rigsby, 1973). This perspective distinguishes among schools on the basis of their predominant value systems and what they imply regarding contextual "press for achievement." The other perspective is that standards and values are conveyed interpersonally by way of individual friendship patterns (Campbell and Alexander, 1965). In schools with large high SES enrollments, for example,

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6 I have in mind here studies which focus on levels of organization beyond the school in trying to understand the reasons for unequal achievement. This would include earlier work on school district differences (Bidwell and Kasarda, 1975) and the recent interest in public sector-private sector comparisons (Coleman, Hoffer, and Kilgore, 1982), both of which I have had occasion to criticize (Alexander and Griffin, 1976; Alexander and Pallas, 1983; 1984).

There may well be good reason for wanting to understand more about both sorts of organizational differences, but the expectation that this will tell us much about why some youngsters do better in school than others is not one of them. In fact, the futility of this was anticipated in the EEO Report itself, which documented that only between ten to 20 percent of the variability in cognitive performance was captured in differences across schools in average performance levels. This sets an upper bound on the influence of any and all specific school characteristics, and it is one of the main reasons the Report turned up so few important school-level predictors of test performance. Most of the variability in student performance resides in individual differences within schools, not in average differences between schools.

This recognition out to inform where one looks for evidence of effectiveness, but it doesn't always work that way. I have estimated that only about one percent of the variance in secondary students' test performance is captured in either school district differences or in educational sector differences. In light of this, it would seem there ought to be more promising avenues to pursue, and indeed there are.

intentions to go to college are widely held, and therefore the likelihood of acquiring close friends who hold such plans is enhanced in such settings, as against in a school where relatively few students are college oriented.

Another theoretical perspective on the way student body characteristics can influence achievement patterns emphasizes social comparison processes. Self-understandings are formed in part by comparing oneself against others in one's surroundings (e.g., Davis, 1966). Such judgments, then, are relative rather than absolute, and hence in the context of schooling are conditional on the mix of other students in attendance. For example, whether one comes to think of one's self as a really good student depends partly on the level of the competition, and this is determined by the overall quality of the student body. Where the competition is stiff, as in highly select schools, students at a given level of ability will perform relatively less well than if they were up against weaker competition. They will get lower GPAs, for instance, and since this weighs heavily in how they regard their own abilities, one would expect adverse effects on such things as academic self-esteem and future goals.

While such dynamics have never been found to produce large effects on achievement (see Hauser, 1969 and 1971), they nevertheless have been observed in many studies. There is evidence, for example, of both social comparison and reference group processes revolving around student body characteristics, that they often operate concurrently, and that they sometimes pull in opposite directions. For example, participation in a high ability environment tends to depress performance via social comparison processes, while participation in a high SES environment tends to boost it by encouraging high goals (Alexander and Eckland, 1975; Alwin and Otto, 1977; Meyer, 1970).

These approaches all involve social-psychological mechanisms through which context impinges on the individual, but here the focus is on implications that flow from the school as a collectivity. Another, rather different perspective has been advanced recently by Barr and Dreeben (1983; Dreeben and Gamoran, 1986) in their research on pupil performance in a sample of Chicago area elementary schools. Their study considers ways in which characteristics of the student body, especially its level of academic ability or preparedness, can influence how instruction is organized. They found that schools and school districts that enroll less able students tend to adopt easier reading series, and that teachers in such settings make slower progress through them, even in what are supposed to be the more able reading groups. Hence, a child at a particular level of readiness would make slower progress in such a setting than in a school with more able students owing to ways in which the quality of the student body constrains organizational process.

A study of secondary schools in inner-city London, England, also has found that the character of the student body can exercise considerable influence over a school's effectiveness. Rutter and his colleagues (1979) considered several facets of what they refer to as student "intake": occupational mix, ethnic mix, academic mix, and behavioral mix, the last having to do with the percent of problem children in attendance. Rutter's study concluded that something akin to a

threshold exists, beyond which the "drag" of less able students is difficult to counteract. They found that academic results are best in schools with "... a substantial nucleus of children of at least average intellectual ability, and delinquency rates were higher in those with as heavy preponderance of the least able" (p. 179).

Rutter's study is distinctive on several counts. First, it considers a variety of educational outcomes, not just school performance. Attendance, behavior in school, and delinquency behaviors were given coequal attention with performance, and I consider this very important as the good accomplished by some kinds of schools may be concentrated in precisely such noncognitive areas. In being preoccupied with test scores and the like, these other aspects of school effectiveness could well be overlooked. The Rutter study is a useful corrective to such narrowness, which characterizes much of the literature.

The methodology of the Rutter study also has distinctive virtues. They elected to study 12 schools. This was sufficient to provide variability within the sample, but not so large that the researchers had to rely exclusively on survey methods. While traditional kinds of survey data were gathered, these were supplemented by intensive interviews and on-sight observations. The final product is an appealing blend of quantitative and qualitative analyses, and conveys the sense that the researchers really have a feel for what life in these schools was like. Let me quote from their conclusions (Rutter et al., 1979, p. 204):

> One of the common responses of practitioners to any piece of research is that it seems to be a tremendous amount of hard work just to demonstrate what we knew already on the basis of experience or common sense. Was the effort really worthwhile? It might be felt that the same applies to this study. After all, it is scarcely surprising that children benefit from attending schools which set good standards, where the teachers provide good models of behaviour, where they are praised and given responsibility, where the general conditions are good and where the lessons are well conducted.
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> Indeed this is obvious but, of course, it might have been equally obvious if we had found that the most important factors were attending a small school in modern . . . premises . . . with a particularly favourable teacher-child ratio, a year-based system of pastoral care, continuity of individual teachers, and firm discipline in which unacceptable behaviours were severely punished. In fact *none* of these items was significantly associated with good outcomes, however measured.
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> Research into practical issues, such as schooling, rarely comes up with findings which are totally unexpected. On the other hand, it is helpful in showing which of the abundance of good ideas available are related to successful outcome.

This passage expresses well what I consider a profound truth about the value of fundamental research. I mentioned above that the social sciences often are dismissed as simply documenting the obvious, but this is countered by numerous examples of its "debunking" contribution, the EEO Report being a case in point. The Rutter quote tells us that such study can also help in sorting out a wealth of reasonable possibilities. In fact, the picture painted by Rutter is well aligned with the

EEO Report in discounting the importance of educational "hardware," in favor of such "soft" notions as teachers' attitudes, the normative climate or ethos that prevails, qualities of administrative leadership, and student intake. This also lines up quite well with ethnographic, case study descriptions of "effective schools" (see Mackenzie, 1983), as well as, at least by implication, with the many studies that demonstrate the near irrelevance of such things as class size (above an impractically low threshold—see Glass, Cahan, Smith, and Filby, 1982) and teachers' qualifications (with the possible exception of their verbal skills—see Levin, 1980; Jencks, 1972; Smith, 1972). There is impressive convergence on these matters across diverse literatures, and I very much doubt that our understanding of them would have progressed to this point in an applied research environment.

Another understanding that I think would have gone undetected absent fundamental research involves the overall contribution of schooling to youths' cognitive development, especially that of minority and disadvantaged youths whose home-based academic resources are not abundant. It once would have been inconceivable for any person of responsibility to question the value of schooling, and certainly the American public generally has long held education in high regard. This confidence was shaken, though, by the EEO Report, which often was misconstrued as indicating a disturbing lack of efficacy for the entire educational apparatus. "Schools make no difference; families make the difference" was the impression (quote attributed to S. M. Lipset in Hodgson, 1973), and it was a hard one to shake once established.

The Report, though, actually has little to say about schoolings' effectiveness, as the issue it addressed was much narrower: the contribution of school-to-school differences in resources to school-to-school differences in student achievements. The Report did not undertake to study school-based sources of individual differences in achievements, about which more will be said shortly, nor did it consider what achievement patterns would look like in the absence of institutionalized schooling. The latter, I think, is the more proper basis for judging whether "schools make a difference," but to put such a thesis to the test is no simple matter where schooling is near universal.

There are, though, extended periods when most youngsters are not in school, and it was Barbara Heyns' inspiration to exploit the natural experiment built into the academic year calendar to inform the question of whether cognitive gains during the school year exceed those during the summer months (taking into account various complicating factors, such as summer school attendance by some youngsters and the time frames covered by the two intervals). Using fall-to-spring test score changes to map school-year growth and spring-to-fall test score changes to map summer growth, Heyns (1978) studied the achievement patterns of fifth, sixth, and seventh graders from Atlanta City public schools over an 18-month period.

Comparing blacks and whites and different family income levels, she found that the scores of white youngsters and of children from high-income families improved year round, summer and

winter. For minority youngsters and children from lower-income households, a very different pattern was observed. During the summer months, when schools were not in session, their achievement profiles were either flat or, in some instances, even declined. During the school year, though, the achievements of these youngsters improved sharply, although still not at a rate commensurate with pupils from more advantaged backgrounds. Nevertheless, the summer-winter contrast for disadvantaged children was quite striking, indicating an important contribution of schooling to their cognitive development, which apparently helped compensate for a lack of effective academic resources in their home and community environments. One implication is that in the absence of formal schooling, the majority-minority and the high SES-low SES achievement gaps would be even larger than at present.

This general pattern has been observed in other studies as well (Heyns, 1987; Murnane, 1975), and our own work (Entwisle and Alexander, In Press (a)) with the BSS data, which covers two summers and two school years, shows even stronger compensatory trends, except in the case of the most disadvantaged black youth. The fact that the Baltimore data involve the first and second grades, while Heyns' study involved the fifth, sixth, and seventh, may indicate something about the crucible nature of the early transition years, a stage of schooling which has yet to receive the attention it deserves from sociologists.

Our analysis also clarifies how family factors enter the picture on a seasonal basis. All family influences were more pronounced during the summer than during the winter, but family SES influences on verbal growth came to light almost exclusively in the summer months. Social-psychological resources flowing from the family, on the other hand, which included such things as parents' judgments of their children's ability levels and their performance expectations, were important to cognitive gains year round, and their importance is consistently greater than that of material supports.

That more material family effects are damped during the school year suggests that schools can help fill in for such tangibles. The year-round importance of social supports, on the other hand, identifies achievement-oriented family press as valuable *apart from other resources of both family and school.* This is potentially quite important, for it tells us that if the commitment is there even economically disadvantaged families can be engaged effectively as agents of academic socialization. This, of course, is the goal of family involvement programs, and our research gives reason to think there is considerable potential in such initiatives.

Once the "schools don't make a difference" mentality is displaced, it is possible to see all sorts of ways in which schooling can help fill the void. I recently have conducted research comparing the test score gains of high school dropouts in the years after leaving school against gains over the same interval of youngsters who remained in school through high school graduation. These studies show that staying in school enhances cognitive skills across a variety of areas (Alexander, Natriello, and Pallas, 1985), and that the contribution is greatest for students who took an academic program

of study while in school (Natriello, Pallas, and Alexander, 1989). This last detail seems sensible enough, as the academic curriculum is more oriented to the kinds of skills covered in standardized tests of the sort used in our research—vocabulary, reading, math, science, etc. Hence, another respect in which "schooling makes a difference" is revealed in the cognitive gains associated with staying in high school through graduation.

At the other end of the schooling cycle, recent indications that preschool compensatory education programs can have long-lasting effects also help fill out the picture. Although this research usually is conducted in an evaluation context, it nevertheless squares well with the accumulating evidence from the fundamental studies just reviewed. The coordinated effort of the Consortium for Longitudinal Studies (Lazar and Darlington, 1982), which entailed follow-up assessments of 11 diverse compensatory education programs, goes a long way toward counteracting earlier impressions that such programs, except under very restrictive conditions, accomplish little of lasting value (e.g., Jensen, 1969; McDill, McDill, and Sprehe, 1969).

Interestingly, the Consortium's conclusions *regarding cognitive benefits* from program participation generally are in accord with these earlier assessments of compensatory education's effectiveness. Any such benefits, it appears, are small and short-lived, the so-called "fade-out" effect. Evidence from the BSS (Entwisle, Alexander, Cadigan, and Pallas, 1987) shows a similar pattern for the effects of full-time versus half-time kindergarten programs—an initial cognitive spurt, which soon washes out, in our data by the end of first grade.

Early education initiatives have been deemed failures on this basis, but in light of what the literature now reveals regarding seasonal learning patterns for disadvantaged youngsters, I'm not sure such a conclusion is warranted. Clearly these kinds of youngsters look to the school for intellectual stimulation, and schools deliver, perhaps not to the extent that we would like, but they certainly help disadvantaged students perform at a higher level of competence than they would otherwise. That the gains of early education do not persist when children move into the regular school schedule may signal not their failure, but, rather, a lack of continuing institutional support to sustain their successes.

Many disadvantaged youngsters need extra attention and resources to do well in school. This, presumably, is what early education provides, and it shows up as making a difference when the alternative is no schooling. But the BSS analysis of seasonal variations in learning patterns shows that regular school programs have a strong leveling effect, and it may well be that the gains associated with preschool program participation blur as the most disadvantaged youngsters make up for lost ground.

For the benefits of early education to persist, it may be necessary to enrich the regular school experience in much the same way that the early education programs themselves represent an infusion of extra time and effort. Our present knowledge base can't distinguish lack of efficacy from lack of

follow-through, and for that reason I think it premature to declare compensatory education a failure even for cognitive criteria.

On other counts, the indications clearly are positive. One study with a particularly long time frame, the Perry Preschool Project (Weikart, Bond, and McNeil, 1978) tracked down students in 1976 who had been enrolled in a series of intensive preschool education experiments, conducted between 1962 and 1967. Compared with control youngsters, the preschool participants had higher high school graduation rates, better employment histories, and lower delinquency rates. And results from the Consortium analysis indicate other noncognitive benefits associated with preschool participation. Participants were less likely to be assigned to special education classes or to be retained in grade, were more likely to give achievement-related reasons for being proud of themselves and rated their performance higher. Additionally, their mothers expressed greater satisfaction with their school performance and held higher aspirations for them.

This strikes me as an impressive compilation of positive consequences, and it reminds us again, as with the Rutter study, that some of the more striking accomplishments of effective educational interventions may be in noncognitive areas. And, too, we are reminded that these won't be appreciated unless someone poses the right questions. There are no guarantees that fundamental studies will get us there, but one advantage in being less bound to immediate concerns is the potential for turning up something important that has been overlooked in the conventional wisdom. Our belated appreciation of the noncognitive benefits of schooling and the compensatory contribution of schooling to the academic skill development of minority and disadvantaged youngsters may well be two such insights.

To this point, we've considered characteristics of schools that seem to affect what transpires inside them and research on schooling versus no schooling as a way of gauging institutional impact. Both thrusts can be understood as outgrowths of an effort in the wake of the EEO Report to reconsider how it is that schools might impact on children's well-being. In wrapping up this section, I must at least mention another line of research that also was conceived in the fallout from the EEO Report. This literature is defined more by its level of analysis than any substantive commonality. It focuses on "within-school" processes, or, in the parlance of the economists, "micro-data."

The EEO Report established that most of the variability in achievements is represented in individual differences in performance levels *within* schools. In light of this, it is altogether reasonable that one should scrutinize the internal workings of schools and the characteristics of the proximate learning environment for clues as to the conditions that help shape performance. The idea here is to get closer to the daily routine of youngsters to ferret out qualitative differences in nominally equivalent settings. They may all be attending the same school, but they're not all having the same experience, and it is differences of first-hand experience that most weigh on academic development.

Dating from the 1970s, quite substantial literatures have accumulated on the following aspects of the proximate learning environment: teacher effectiveness (Alexander, Entwisle, and Thompson, 1987; Bossert, 1979; Brown and Saks, 1975; Murnane, 1975; Summers and Wolfe, 1977; Winkler, 1975); principal effectiveness (Bossert, Dwyer, Rowan, and Lee, 1981; Blumberg and Greenfield, 1980; Leithwood and Montgomery, 1982); task and reward structures (Epstein and McPartland, 1979); instructional organization (Barr and Dreeben, 1983; Dreeben and Barr, 1988); time on task (Karweit, 1976a; 1976b; 1985; Wiley, 1976; Wiley and Harnischfeger, 1974); ability grouping (Eder, 1981; Felmlee and Eder, 1983; Gamoran, 1986; Rowan and Miracle, 1983; Sorensen and Hallinan, 1986); and high school tracking and course-taking patterns (Alexander and Cook, 1982; Alexander, Cook, and McDill, 1978; Alexander and McDill, 1976; Alexander and Pallas, 1984; Davis and Haller, 1981; Garet and DeLany, 1988; Gameron, 1987; Gameron and Berends, 1987; Heyns, 1974; Lee and Bryk, 1988; Pallas and Alexander, 1983; Rehberg and Rosenthal, 1978; Rosenbaum, 1976; Vanfossen, Jones, and Spade, 1987).

What ties these literatures together is their concern with educational process at the level of first-hand experience. They still are fundamentally concerned with organizational issues, but it is the organization of daily experience and the structure of the immediate learning environment that is at issue. To do justice to these materials would itself require a paper-length treatment, and I introduce them at this point simply to have them on record. They represent an important thrust of current research on organizational process and educational inequality. My purpose in tracing their lineage back to the EEO Report is to draw attention to the cumulative and self-correcting nature of fundamental research when it is working well. I think we've learned a great deal about the workings of schools through these initiatives. They are, collectively, an eloquent testimony to the way knowledge can advance through basic study.

Discussion

The materials reviewed in the preceding section comprise a sociological specialty known as the field of "educational stratification." As described by Hauser (1970, p. 104), its agenda is "the identification and interpretation of mechanisms linking social origins, performances in the education system, and adult achievements." In recent years, research couched in these terms has focused increasingly on the workings of schools, and especially on their internal workings, to the point where the origins-destinations connection often is obscured. This, though, is the heritage of such inquiry, and in learning about socially patterned educational inequality our understanding of how stratification systems are maintained also is furthered. But my purpose here is to consider education policy and how research which makes no pretense to change things might be of service to those who want to make schools better.

Whatever value resides in such enterprise obviously follows from the understanding it affords, and it is the context of understanding that is important. Research on schools from a

stratification perspective locates them in the larger social matrices within which they are embedded. It also looks to propositions from outside the educational arena to inform happenings inside—insights from social-psychology and social structure and personality with respect to the student as an academic performer and insights on organizational structure and functioning with respect to schools as complex organizations. This is another kind of embeddedness, one that tries to comprehend what transpires in schools as contextualized representations of principles that themselves are contextless. Interest in interinstitutional linkages follows from the stratification connection; invoking general propositions is near inherent to the fundamental science approach. Together they force a kind of cross-disciplinary integration that removes educational concerns from the exclusive province of educationists.

I think this is good, and it is good too that in not being preoccupied with changing things the fundamental research perspective is not bound to consider only things that are changeable. What is considered fair game for intervention is not fixed, and things that are inviolate or impractical today may be tomorrow's target of opportunity. By limiting itself to "alterable variables" in the current climate of technology and opinion, need-driven, mission-oriented applied studies risk missing opportunities to discover "alterables" for the future.

Research on "school climate" or "ethos" is a case in point. Hard-nosed types never have taken to such fuzzy notions, but fundamental research took up the challenge to see if schools' value systems could be measured reliably, to see if schools could be distinguished on this basis, and to see if such differences mattered for students. The answer was yes on all counts. I would guess that at the beginning hardly anyone took these issues very seriously, and if pressed few would have thought it at all practical to transform schools whose value systems were not especially conducive to academic excellence into ones that embodied a so-called "press for achievement." But the research progressed, establishing that there was indeed something to these ideas. And with that encouragement, some took up the challenge of developing strategies for putting ideas into practice (e.g., Brookover et al., 1982).

The "school climate" experience represents perhaps the ideal type of knowledge transfer, where fundamental research sets the stage, development efforts pick up on its insights, and better schools follow. But this is the exception, not the rule, and the case for fundamental research ought not rest on the occasional development breakthrough. Fortunately, it doesn't have to. By way of conclusion, I now suggest some further possibilities.

First, fundamental research can look outward from the educational system, helping to locate its role in the larger social system. This is reflected in studies that show how educational inequalities contribute to socioeconomic inequalities, how educational opportunity can be the springboard to later opportunities, and how schooling as an institution impinges on various aspects of student development, including, importantly, cognitive development.

In examining schooling as an institution, fundamental studies have the latitude to pose questions that go to the very heart of the enterprise. These may not always be flattering or comforting, but it is important that there be a nonpartisan, independent constituency that can tell the emperor when he's underdressed. Applied studies, by virtue of their mission, are near servants of the system. Their responsibility is to help fix things. They tend to be locally oriented and often are conducted under the auspices of the education bureaucracy itself. By virtue of both mandate and sponsorship, they are constrained to accept as given the parameters of the situation and to frame their work within them. This limits the kinds of questions they pursue, and correspondingly, the kinds of answers they provide. There is advantage, I would argue, in distancing oneself from both vested interests and immediate concerns, and fundamental research at least has the potential for such detachment. Studies of schooling and inequality, for example, afford a balanced picture of where the institution fits in: it is neither so potent, nor so impotent, neither so good, nor so evil as many would have us believe. Such a guarded assessment may not arouse passions, but it is accurate, and it is important, and it comes from fine-tuning the issues and evidence through many years of basic study. Fundamental research, then, can help us understand what education does, and does not do, for the individual and for society. This is the backdrop to practice, and it is basic research that tells the story.

Second, fundamental research looks within the schools, teaching us more about how they work. This can help us understand the scope and nature of things considered problems, and often also suggest strategies for school improvement. Research on the patterning of educational inequalities reveals persistent disadvantages in academic performance and school attainments that are in some instances quite severe and in all instances disturbing. But the analytic approach of the basic research perspective embeds its description of such patterns in an interpretive framework, and this can have considerable value.

In a world of multiple disabilities that tend to go hand in hand, impressions can be misleading. Nonexperimental fundamental study is basically a strategy of disciplined observation. It takes information on happenings in the messy real world and organizes it around theoretical or conceptual concerns that then constitute the interpretive backdrop. The "ascription" versus "achievement" distinction in studies of educational inequality is one such organizing logic. Such inquiry takes as its point of departure socially patterned differences in school performance, such as those revolving around race/ethnicity, gender, and/or family background status. It then attempts to identify which of these lines of demarcation are the more fundamental, in the sense either of limiting performance independent of the others or of being related to performance independent of so-called merit considerations.

It is through this filtering that differences come to be understood as inequities, that the more severe sources of educational disadvantage come to light, and that the range of likely contributing factors is narrowed. These features all are illustrated in the literature reviewed above, which has helped clarify and counteract common misunderstandings. Such "disciplined observation" is never

complete, or completely authoritative, but the approximations it affords can be both useful and close. It is in these respects that the fundamental science approach can help us understand the "scope and nature of things considered problems."

Its other "inward looking" contribution is in helping suggest strategies for school improvement. The possibilities here are practically limitless, and fundamental research on school effectiveness can offer guidance as to which hold the greatest promise. By now it certainly ought to have disabused us of the notion that simply throwing money at schools is the solution. The Equality of Educational Opportunity Report taught us that school differences in resources, facilities, curricula, and staffing are only a small part of a very large problem, and this has been confirmed time and again in subsequent research on various "consumables," including such things as upgraded teachers' credentials and reduced class size. This isn't to say that the solutions, when they are understood, will be cheap, only that they are not likely to involve things that are either obvious or easy to implement.

We know enough now not to expect to uncover a single, prepotent "smoking gun." A more realistic hope is to identify several, perhaps even many, sources of leverage that could be pursued in concert. A supportive school ethos, organizational constraints and social-psychological dynamics that follow from the mix of students in attendance, and the attitudes and expectations of teachers all play an important role, along with numerous details of how resources are organized and deployed within schools and classrooms. Each of these individually might make only a small difference, but the cumulative effect could well be striking. As an agenda for change, the lessons from fundamental research offer no quick fix, but schools can, and do, accomplish considerable good, especially for those who most need the intellectual stimulation they provide. They can do even better, though, and basic research can help guide the efforts of those responsible for improving practice by distinguishing more promising from less promising avenues. In fact, I believe its record in this regard already is impressive, if people only would take it seriously.

Third, and finally, fundamental research can look inward and outward *simultaneously*, a breadth of perspective that can be very useful. School performance is governed by a multiplicity of interdependent forces involving the child as performer, his or her immediate interpersonal environment, and the home, school, and community as contexts for personal development. Research can pick this bundle apart to focus on some particularly salient aspect of it, but it also needs to respect the totality if the pieces are to fit together. Applied studies are well suited to hone in on some element of practice in an evaluation context, but the big picture is more the province of basic research, if only because its scrutiny of things educational is couched in terms of the problems and perspectives of the parent discipline. It is the very nature of such inquiry that it has a broader or more encompassing purview, the field of educational stratification being a case in point.

At issue here are the implications of factors located outside the schools for what transpires inside them. I see two main advantages in doing research that draws out interdependencies across

institutions. One involves lessons learned about the constraints under which schools labor. That's the bad news, although it can have salutary consequences if it encourages a more realistic attitude as to what reasonably can be expected from school reform. The other is potentially more constructive. It involves identifying extra-schooling resources that might be enlisted in the cause.

The two advantages actually are opposite sides of the same coin. In the educational stratification literature, they are found in studies that link in-school performance to attributes of the students and their friends that are external to the school, and to various characteristics of their families and their communities. All these constitute "outside influences" relative to the learning resources that schools control, and they can be either part of the problem or part of the solution, depending on whether they pull against or with the school's agenda.

The constraints are many and potent. This is yet another respect in which lessons from the EEO Report have stood the test of time: differences in family circumstances and background are much more implicated in patterns of educational inequality than anything having to do with schools. The list is all too familiar, and all too long: poverty conditions, low levels of parents' education, single-parent households, large sibships, in many instances minority group status, language handicaps, poor nutrition, lack of support for conventional goals and values, and on and on. And as a result of growing up in such conditions, youngsters present themselves to the schools already lacking in many of the qualities that make for success. Our Baltimore data show, for example, that differences in cognitive skill levels and in qualities of temperament or personal maturity already in place at the time children begin school are powerful determinants of success in first grade. This shows up in both teacher-assigned marks and test score gains. And, of course, the conditions that conspire to produce children lacking in the requisite skills and qualities of character at the time of school entry continue to weigh on their development even as the schools struggle to counteract them. A pattern of low or underachievement tends to feed upon itself, and once established it is exceedingly difficult to turn around. We know this from evidence on the persistence of achievement trajectories from the earliest grades on.

These are hardly startling revelations, as it was precisely to counteract such baggage of personal background that the compensatory education movement was conceived. Yet to know there is reason for concern is not the same as understanding the details, and it is fundamental research, by and large, which has carried forward the work. Which risk factors are of greatest consequence, are material or cultural conditions of the family most problematic, on which aspects of personal development do these various disadvantaging conditions most impinge, and do things work differently for different kinds of youngsters?

Having the answers doesn't itself tell us how to turn things around, but it at least identifies where the need is greatest. Of course, the particulars will vary from place to place, while basic research paints its picture broadly. It informs our understanding of the outside conditions that

impinge on schooling generally, and this can at least alert those responsible for program planning at the local level to potential problem areas.

But this information has implications that go beyond simply troubleshooting. Schools have been assigned responsibility for correcting society's mistakes, and this is a weighty burden indeed. Research of this sort helps us understand the magnitude of the task, and this helps place the school's responsibility in perspective. Perhaps schools are failing when large numbers don't finish high school, are barely literate when they exit the system, and in various respects perform below what is considered their potential. But schooling doesn't take place in a vacuum, and this research reminds us that to a very substantial degree such failures originate in conditions outside the system. It may well be proper to expect schools to compensate for problems not of their making; this is a political/social judgment, not a scientific one. But if this is the standard then it is important to understand that the problem is not so much deficiencies in the educational system as it is the challenge of counteracting the failings of other social institutions, that these failings are numerous and severe, and that the resources schools bring to bear on the task are meager relative to the potency of countervailing forces. None of this absolves schools of their delegated responsibilities, but it can help various constituencies understand more properly exactly what those responsibilities entail. In tackling problems indirectly rather than at their source, the challenges are that much more severe.

Again, it is through "big picture" fundamental research that this story gets told. And the same knowledge base that teaches us about how problems in the outside world impinge on happenings in the schools also identifies sources of positive spillover. These too are potent and multifaceted. Many, of course, are simply the opposite side of the disadvantaging characteristics mentioned above—comfortable circumstances, a stable supportive family, and so forth. That the good and the bad exist side by side is the main reason meritocracy in the schools helps perpetuate social inequality.

But we also know that the "bundling" of advantaging and disadvantaging conditions is only approximate, and in many instances surprisingly loose. This implies various "windows of opportunity" that school improvement programs might be able to exploit. Supportive parental attitudes, for example, constitute valuable resources even in economically disadvantaged households. But they are not as useful as similar sentiments among the well-to-do, and this is troubling. Perhaps programs could be designed to more effectively exploit the good intentions in such families, helping empower parents as agents of positive academic socialization. The research evidence tells us there is considerable potential in such initiatives, and various parent involvement programs seem to be having success in drawing it out.

We also know that many youngsters raised in home and community circumstances that would seem to put them "at risk" for academic failure somehow manage to rise above those conditions. They enter school with good readiness skills, an inclination to do the things expected of them, and an inquisitive, positive outlook. And if these qualities can be sustained, they tend to

pay off, much as they do for more advantaged youngsters, the only difference being that they are more commonplace among the latter. It could occasion an extraordinary turnaround in scholastic prospects, even life prospects, if we understood better the sources of strength in families that are materially poor or otherwise highly stressed yet somehow overcome the odds.

In light of what we know about the importance of the family and of individual differences of competency and disposition for school success, I suspect that the major breakthroughs in redressing problems of low achievement and underachievement will come either from outside the schools or from forging more effective home-school partnerships. And I suspect too that it will be fundamental research that points the way. This is because its scope spans institutional boundaries, and it is in the intersection of family process, school process and home-school linkages that answers are to be found.

These, then, are the several respects in which fundamental research serves the interests of those concerned with school improvement. Beyond the occasional instance of successful knowledge transfer, there is value in understanding the role of schooling in society, value in understanding what it is about schools that makes a difference under prevailing conditions, and value in understanding how outside influences affect what happens inside schools. These several points were illustrated by reference to the literature on educational stratification, and especially on the patterning of achievement and attainment inequalities. By its very nature, basic research is more concerned with general principles than with the particulars of this or that situation. It is for this reason that the picture it paints is drawn in broad strokes, and rarely do its lessons assume the form of an implementable agenda for change. But understanding the conditions of education is, it seems to me, a necessary backdrop to well-conceived interventions and policies, and basic studies can identify promising avenues to be pursued by those responsible for improving practice.

Evaluation studies and policy research are skewed toward local conditions and immediate concerns; they also are skewed toward policies considered practical and politically acceptable. For these reasons their generality is suspect, and their coverage limited. We certainly need high quality research that serves the interest of practice, and it is one of the more significant accomplishments of the last two decades or so that an applied research infrastructure has been put into place. But the big picture also needs to be understood, without regard to either practicality or popularity. This is the kind of knowledge fundamental studies are intended to generate, and they do so by applying to things educational propositions from the basic science disciplines regarding human and organizational behavior. This is another way of saying discipline-based fundamental research, and it remains an important strategy for understanding the working of schools.

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COMMENTS FROM ANOTHER "IVORY TOWER"

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I was delighted to read this paper, which combines clarity of exposition with a felicity of style only too frequently absent in the writings of many contemporary sociologists. In one sense, my task as discussant has been made more difficult since there is so little with which I find myself in disagreement and thus I am obliged to expand upon more general issues and perhaps fill in part of a broader scenario within which the research tradition so ably discussed by Dr. Alexander must be placed.

Since a degree of biographical detail seems to be in order, let me begin by emphasizing some of the remarkable parallels between his "starting points" and my own. We are both sociologists and share the view that research in the sociology of education must be firmly rooted in the concerns of the parent discipline. Neither of us is an educationist and neither of us has had "much of an intrinsic interest in educational issues," though I must confess that, in my case, that interest has been more substantial where my own children have been concerned. We are all thoroughgoing egalitarians save for the fate of our own offspring and, like the rest of us, my particularistic interests in taking care of my own comes into conflict with the "universalistic" interests of society.

As with Alexander, my own interests in education have stemmed from a long-standing research concern with stratification in human society, and like him my long-term intellectual commitment has been to fundamental rather than policy-oriented research. It often turns out, however, that serendipitous findings often emerging out of our major research agendas are conceived by others to have substantial, if indirect, policy implications, and I am sure that he, like I, has become involved in issues of policy. I believe it is incumbent upon both of us to explain to the policymaker anxious for the odd "quick fix" just what our findings might or might not imply in the policy arena since our shared intellectual caution is sometimes interpreted as little more than intellectual timidity.

There are, however, two or three respects in which we might differ in emphasis. The first of these concerns the broader intellectual traditions within which our own studies have taken place. I took my first degree in sociology at the London School of Economics in 1948, and on coming to the United States in 1949 for a year's postgraduate study I was surprised to find that the sociology of education in this country seemed to be overwhelmingly concerned with issues of classroom management, school organization, and peer group behavior, with little emphasis on the broader macrosociological issue of the relation between education, social stratification, and processes of social mobility. Indeed, at that time the only work that seemed to address the issue explicitly was

Warner and Havighurst's *Who Shall be Educated*, published as late as 1944. By contrast, what contemporaneously passed for the sociology of education in the United Kingdom was almost totally concerned with the issue of education and social equality with a tradition of empirical research (often to be found in the Reports of Royal Commissions) dating back into the 19th century. Moreover, I was impressed in 1949 by a widespread American belief in the role of education as the great "equalizer" since British and, indeed, European traditions were far more sceptical and tended to view educational systems as largely reflecting the extant social order and indeed legitimating it. Doubtless, this sceptical view reflected, in part, the greater influence of Marxist traditions, but in the British case it certainly had substantial non-Marxist and non-Weberian roots.

In fact, the great impetus to U.S. research in the relationship between education and human inequality really stemmed from the Brown Decision of 1954 and only took tangible form in Coleman's work in the mid-1960s. As is so often the case, an earlier optimistic view of the equalizing function of education was then followed by a spate of quasi-hortatory literature (supported by some empirical evidence) which argued that far from being the instrument of desirable social change, schools were nothing more than the instruments of "class-domination" and "the social reproduction of inequality." Only major structural transformation and not educational reform, it was argued, could change the immutably unjust nature of U.S. (capitalist) society. I now shudder to think just how many American students emerged with the idea that the work of Bowles and Gintis or Randall Collins said all that needed to be said about the relation between schooling and stratification (see for example, Bowles and Gintis, 1976, and Collins, 1974). Yet at the same time, their more radical stance did constitute something of a useful antidote to the Pollyannaish view that all that was needed was a dose of educational reform. The radical critics, however, tended to throw the baby out with the bathwater, and Alexander's paper shows how a viable research tradition has grown out of an initially partly ideological debate. While conscious of the broader structural constraints within which schools operate, that research tradition attempts to elucidate just what schools *can* do in redistributing the pattern of life chances of individuals and groups and what are the potential "levers" that might lead to significant policy outcomes. The schools can't do everything, but it is equally evident that they can do *something* and thus, in view of the very different traditions from which we began, it would seem that both of us exhibit a degree of consensus concerning both the limits and potentials of educational reform.

A second respect in which we might differ in emphasis is the degree to which more explicitly comparative or cross-national research might cast greater light on both theoretical and policy issues. Understandably, Alexander's work has been heavily focused on the United States, though I would venture to suggest that some evidence already indicates that many of the research conclusions reached in the U.S. context will be replicated in other "advanced" western nations. Even here, however, it is likely that local variations will cast light on the extent to which different formal selection procedures, for example, will have some effect on the relation between social, gender, or ethnic provenance and educational attainment. Cross-national research in this context provides us

with an opportunity to examine the various outcomes of different national educational strategies and perhaps might point to a broader number of potential policy options.

Moreover, I think we can cast our comparative net even more broadly. After all, when we compare the relation between education and social opportunity in advanced nations, we are examining schools as essentially "homegrown" or "indigenous" institutions. To be sure, the advanced nations undertake a little cross-national educational borrowing from time to time, but essentially their schools have evolved in close relation to their broader social structures and are rooted in local culture and history. What can we learn, therefore, from studying nations or societies where formal schooling, as we know it, is an introduced and initially alien phenomenon, as has been the case, for example, in many of the former colonial territories of Africa or Asia? At the risk of oversimplification, we might argue that in Europe and North America the schools are in some measure dependent variables that reflect the broader socioeconomic structures of society, while in many of the so-called new states they constitute a powerful independent variable that leads to processes of class formation and new patterns of socioeconomic inequality. At the risk of boring the more policy-oriented among us, comparative research in some of the less-developed nations will allow us to address major theoretical issues concerning the relation between education and stratification which, after all, constituted the starting point for Alexander's whole research agenda. Yet even in this case, research may not be without potential policy implications. For example, one of the most common findings in the less developed countries has been that, by and large, the influence of social background on educational attainment has been significantly less than in the developed nations and the effects of schooling commensurately greater. There is, indeed, controversy concerning what causes this apparent "reversal" of fairly standard findings generated in developed countries, but once again the broadening of our research endeavors may have long-term policy implications.

Finally, let me broaden the scope of our discussion in another direction. Very properly, Alexander's paper deals with the relation between in-school and out-of-school factors and subsequent educational attainment. He is less concerned with the extent to which such attainment predicts subsequent occupation and income as indicators of more general "social status." Yet this relationship constitutes the heart of the problem. Quite clearly, our concern with equality of educational opportunity stems directly from the widespread belief that level of educational achievement *is* a major predictor of an individual's subsequent status. Would we be so exercised over educational issues if it were shown that educational attainment was a poor predictor of final status, and do we realize that insofar as education becomes more evenly diffused in a society in terms of both quality and quantity that *ceteris paribus* it must become a weaker predictor of such status?

Alexander thus raises an important caveat in his second footnote when he observes that "Education is only moderately strong as a predictor of income or earnings differences altogether"

and that "while educational inequalities certainly are implicated in economic inequalities they are hardly the entire story...closing the educational gap would be expected to make an important dent in persistent economic disparities, but it is not the 'great equalizer' that many would like to believe."

Just how substantial that "dent" might be may well rest on a variety of factors that are exogenous to the educational system. Thus, status attainment models show us that one's own education is the strongest *single* predictor of individual status, but in aggregate terms movements up the scale are largely a function of rapid rates of structural economic change. In other words the equalizing role of education is likely to be very different in periods of rapid as opposed to minimal economic growth. This seems to me to be the kind of crucial "structural constraint" to which Alexander alludes.

To use an example from the less-developed world, it is apparent that in some "new states" massive increases in the provision of education have been conjoined with negligible rates of economic development. In these circumstances greater equalization of educational opportunity has led, in effect, to a ratcheting up of the minimal educational credentials required for occupational access. At an earlier period a given level of educational attainment provided occupational opportunity, but currently this is hardly the case and under such circumstances there is some evidence to suggest that ascriptive factors such as ethnic provinence or antecedent social status are playing an increasing rather than a diminishing rose as determinants of final occupational or income outcomes. This has been referred to elsewhere as the "achievement suppression" syndrome (Lin and Yauger, 1975).

One would hardly suggest that such a depressing outcome is likely to emerge in the United States, but we would emphasize that any redistribution in the pattern or life chances for individuals and groups that might be effected through educational change would be of less significance in those societies where aggregate opportunities are limited. Thus Alexander's discussion is rather like describing a play whose success is likely to be as much determined by the size of the stage and the nature of the backdrop as it is by the quality of the narrative. I suspect that this point has been implicitly recognized, for example, by that segment of black leadership in the United States which has argued that the most serious problem confronting the black community is jobs, not education. That view may sound simplistic, but perhaps it recognizes that the consequences of educational reform will fall far short of popular expectation unless reform takes place in the context of an expanding economy. Thus, the radicals were correct in asserting that educational reform might be ineffective except in the context of structural change. Where they were incorrect was in affirming that such structural changes implied a move towards a socialist economy. To the contrary, I would argue that greater social equality requires that educational reform should take place in the contest of a dynamic market-driven (capitalist) economy, wherein a serious assault is also made upon the widespread persistence of discriminatory practices against minorities. In other words, the

enhancement of educational achievement must be paralleled by policy efforts designed to achieve greater flexibility and equity in the occupational arena.

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APPLIED AND BASIC RESEARCH IN THE SOCIOLOGY OF EDUCATION: COMMENTS ON KARL ALEXANDER'S "IN DEFENSE OF IVORY TOWERISM"

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Papers discussing relations between applied and basic research are usually instances of special pleading. They list the advantages of one over the other and explain why we ought to be doing more of one than the other. In a forum like this one, designed to bring together practitioners of different crafts, the special pleading may acquire a sort of hybrid character. One often finds applied researchers arguing that their work makes theoretical contributions to the basic discipline and basic researchers, like Karl Alexander, discussing the potential practical applications of their work. In other circumstances, perhaps, when they are among their own kind, basic and applied researchers may revert to type and question one another's foci in less ambiguous ways. This does not mean that basic and applied researchers are wrong or duplicitous when, depending upon the circumstances, they stress different aspects of their work. But it does make it difficult to nail down precisely what is at issue.

After listening to today's discussion one could easily be led to conclude that the two kinds of research are pretty similar after all, and that if only basic and applied researchers would cease their carping and posturing we could attain the long-desired unity of theory and practice.

It will never happen. Even people who are specifically engaged in bridging the gap between the two are seldom completely successful. For example, basic researchers teaching in professional schools can rarely avoid clashes with their more practice-oriented colleagues, while the latter often find their discipline-based coworkers maddeningly insensitive to the demands of training practitioners. The gap cannot be bridged, at least not permanently, because the two really are different occupational groups with different vested interests. Not the least of these vested interests is funding. As William F. Whyte (1982) and Howard Freeman and Peter Rossi (1984) have pointed out, in an era of declining budgets for research, sociologists, including sociologists of education, will have to compete with other disciplines and among themselves for governmental and foundation support.

But the differences are deeper than mere rivalry over grants, jobs, and lucrative consultancies. To illustrate, I would like to focus a moment on one of Karl Alexander's points about the contributions of basic research to educational practice. When basic researchers try to apply their findings to practice they often engage in nay-saying, debunking, and whistle-blowing—in telling

us what won't work rather than what will. This negative message—even when offered in a positive spirit—is seldom welcomed by individuals pressed to figure out how to improve practice. Whether it is Philip Foster explaining that vocational education will not create jobs, or James Coleman demonstrating that most achievement differences are not attributable to differences between schools, or Christopher Jencks contending that more books in ghetto school libraries will not reduce poverty, or Karl Alexander showing that research findings on socioeconomic inequalities in education will not help us understand gender inequalities in education—the message seems relentlessly negative.

Why? There are at least two reasons. First, some basic researchers clearly get a lot of pleasure out of tweaking policymakers' noses by pointing out that favored solutions will not work; their pleasure may come from getting revenge on those they feel society takes more seriously than themselves and/or from the opportunity to assert the "purity" of basic research. Second, and more substantive if not more important, it is only possible to *prove* that something is *not* so. Whether one thinks of the matter epistemologically and calls it falsification or methodologically and calls it the null hypothesis, the message is the same: basic research is very often going to be of help only in eliminating blind alleys. While that is no small contribution, applied researchers can seldom afford to work mainly to narrow the range of reasonable alternatives. They usually want more direct ways of finding positive recommendations. Their constituents require suggestions more immediately useful than: "don't try that; it won't work."

What all this illustrates, I think, is that the key difference between applied and basic research (and within various types of each) is the audience or constituency for the research. For applied researchers, the audience might be a state education department, a teachers' union, a school district, a civil rights organization, or a legislative committee. For basic researchers, it might be subscribers to a scholarly journal, an organization of professional colleagues, a funding agency, or a tenure committee. Scholarship tends to fragment along the lines of the audiences for which researchers tailor their results.

It comes as no surprise to hear that ours is an era in which information is increasingly important to decision making. And as James Coleman (1976) has pointed out, in a pluralistic democracy different interest groups are likely to obtain the information they seek from different researchers. It is hard to make a case that certain of these groups or certain of the motives of researchers working for them are somehow "pure" and the others tainted by self-interest. Rather, a pluralist conception of research implies that there are *legitimate* conflicting interests. As Lewis Coser (1956) argued some time ago, conflict can be functional for society as a whole. Or as James Madison argued rather longer ago (in the *Federalist Papers* number 51) we are only safe as long as there is a "multiplicity of interests" no one of which dominates. This, I would argue, is as true of research as it is of civil liberties. Conflict is functional—at least limited conflict in controlled settings.

In any case, we will never achieve the ideal state of Mannheimian intellectuals floating free above the conflict of interests and doing research equally relevant to all constituencies and equally important to applied as well as basic problems. Rather, applied and basic researchers, policy makers and social scientists need to tolerate one another enough to be able to get together frequently and vigorously argue their respective cases. Thus, each may, without compromising fundamental principles, get the maximum benefit out of their conflicting interests and make the maximum contributions to the work of the others.

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